



Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at <http://about.jstor.org/participate-jstor/individuals/early-journal-content>.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

XVII. *Reflections on the Communication of Motion by Impact and Gravity.* By the Rev. Isaac Milner, M. A. Fellow of Queen's College, Cambridge. Communicated by Anthony Shepherd, D. D. F. R. S. and Plumian Professor at Cambridge.

Read Feb. 26, 1778. **T**HE theory of moving bodies was little understood by the philosophers who lived in the sixteenth century. They observed, that a body, once put into motion, continued to move for some time after the force was impressed; but they argued very strangely from this ordinary phænomenon. Far from considering the air as a resisting medium; they supposed with ARISTOTLE and the ancients, that it was the perpetual influx of the parts of the atmosphere which continued to urge the body forward and preserve its motion. When a body is projected in any direction inclined to the horizon, the gravity of its parts is always observed to bend the direction of its motion into a curve line; and because this gravity remains invariably the same, whatever the force of projection be, in very swift motions, the figure described may approach very nearly to a right line.

This

This last circumstance induced some of the philosophers we are speaking of to believe, that a cannon ball, for instance, always moves in the same straight line till its velocity is entirely destroyed; and that afterwards it descends towards the earth in a direction perpendicular to the horizon. Others thought they mended the matter by suspending the action of gravity for a certain period only; by allowing the latter part of the path to be curvilinear; and lastly, the body to descend to the earth in a straight line, as in the former case. We, in these days, who have seen the gradual improvements in mechanics from time to time, are not surprized, that men, in the infancy of that science, should have embraced absurd and ridiculous principles: we rather wonder, how the author ^(a) of the notion just mentioned was able to form any just estimate of the horizontal ranges of projectiles, and to discover their maxima. Whether by conjecture, or probability of induction, we are unable to determine; but so it was, TARTALEA affirmed, what has since been found true upon unexceptionable evidence, that the amplitudes of projectiles upon the horizon are always greatest when the angles of projection are equal to 45° . But the praise of this discovery, as well as whatever else relates to the accelerated motions of bodies near the surface of the earth,

(a) NICH. TARTALEA.

is justly due to the incomparable GALILEO. The theory of mechanics had received no considerable improvement since the time of ARCHIMEDES, when this surprizing genius appeared in the former part of the seventeenth century. He discarded the peripatetic philosophy; explained the whole doctrine of accelerated motion and of projectiles: in a word, he so much exhausted the subject, that the best treatises we have at this day are little more than a repetition of GALILEO's discoveries.

This philosopher, as far as we know, never attempted to investigate the laws by which motion is communicated from one body to another. The celebrated DES CARTES is the first we hear of who gave any attention to the subject; and the result of his enquiries is what might reasonably be expected from so whimsical and romantic a genius; he blundered in this, as in all other cases, where he was not confined to pure mathematical reasonings. Our countryman, Dr. WALLIS, made a real progress in this science, by discovering that fundamental law in the communication of motion, *viz.* that action is equal to re-action, and always in contrary directions: WREN, HUYGENS, confirmed the same thing; and the whole theory of the collision of bodies, and their mutual actions upon one another, seemed to be advancing fast towards perfection.

But

But a new opinion was now started by M. LEIBNITZ concerning the forces of bodies in motion. The force of a body in motion and its momentum had hitherto been considered as synonymous terms, and had alike been measured by the quantity of matter and velocity conjointly. On the contrary, LEIBNITZ and his followers affirmed, that the force was proportional to the quantity of matter in the moving body and the square of its velocity. It is needless to relate all that passed on both sides: so material an opposition in sentiment necessarily produced very warm contention; and, as it generally happens in other disputes, we do not hear of any conviction being produced on either side.

After surveying the arguments of the disputants, it is not easy to say, whether the agitation of the question before us has contributed to retard or advance the progress of truth and science. On the one hand, many ingenious experiments have been made, many curious problems invented and resolved, which probably would never once have been thought of by men who were in the pursuit of truth in a more cool and deliberate way: and, on the other hand, it may justly be affirmed, that the violence of prejudice and party-spirit has so much clouded the reasonings of the best writers, that we sensibly feel their influence to this day. I need not dissemble:

it is a serious persuation, that the laws by which motion is communicated are still very materially mistaken by sensible persons, that induced me to throw together the following hints, and to lay them before the Royal Society. The right understanding of these laws is of the last importance in practice: the good or bad success of some very expensive projects has depended upon it; and certain excellent artists have been disappointed in the execution of their plans, and unable to reconcile the apparent contradiction between theory and experiment. From the length of time, which has elapsed since LEIBNITZ first advanced his new opinions, and the abilities of the philosophers who engaged in the contest, one might have expected, that the whole matter would long before this have been cleared up in a satisfactory manner; especially when we consider, that the communication of motion from one body to another is what every moment happens before our eyes, and that particular experiments are made in this doctrine with the greatest simplicity and convenience. This part of rational mechanics however is not yet generally understood, as we may fairly presume from the difference of opinion which still subsists among the learned. I freely own, it appears to me, that no new experiments are wanting; no new geometrical reasonings or constructions: the improved parts of geometry

metry have been already applied to the theory of motion in numberless cases, and a variety of well attested experiments have been clearly explained to us by authors. The laws of motion, in certain cases, are incontestable, and no author of eminence contradicts them: it is from a mistaken application of these laws that a difference of opinion has arisen. It is obvious, that the laws of motion, as described by Sir ISAAC NEWTON, may, in a certain sense, be founded on experiment; and yet, if they are extended to cases where they cannot be applied, the conclusions must still be erroneous. My design in these pages is to point out distinctly what is real in this difference of opinion from what is merely verbal, and to explain the causes of it. This, which perhaps will appear to have never been done with sufficient precision, seems to be the most effectual way of preventing mistakes. Geometry and algebra will lead us wrong, if our principles are ill founded: experiment itself, if we are not extremely careful, will deceive us in forming a general deduction, or what is called a law of nature. The controversial writings of the most able authors will embarrass and perplex our judgements; but when we have once discovered the grounds of their mutual mistakes and misapprehensions, there is reason to think, that we

shall both understand the subject better than we did before, and be more on our guard for the future.

The first law of motion, as expressed by Sir ISAAC NEWTON, is unexceptionable: nobody denies that a body perseveres in a state of rest or uniform motion in a right line, till affected by some external influence. It is the third law of motion which has produced all this confusion and perplexity. “*Actioni contrariam semper et æqualem esse reactionem: sive corporum duorum actiones in se mutuo semper æquales et in partes contrarias dirigi.*” These words of Sir ISAAC NEWTON convey to us as clear an idea as can possibly be conceived with so much conciseness. It must however be confessed, that his illustration is not so very perspicuous^(b). To say, that when a man presses a stone with his finger, his finger is equally pressed; and when a horse draws a stone by a cord, the horse is drawn equally backwards towards the stone; is a most indistinct and popular way of speaking, and can never make evident what was before not understood.

(b) *Quicquid premit vel trahit alterum, tantundem ab eo premitur vel trahitur. Si quis lapidem digito premit, premitur et hujus digitus a lapide. Si equus lapidem funi alligatum trahit, retrahitur etiam et equus (ut ita dicam) æqualiter in lapidem: nam funis utrinque distensus eodem relaxandi se conatu urgebit, equum versus lapidem, ac lapidem versus equum; tantumque impediet progressum unius quantum promovet progressum alterius, &c. NEWTON Princip.*

Some

Some useful writers, who have copied after Sir ISAAC NEWTON, have talked in the same way; and only increased the ambiguity by being more diffuse. Mr. MACLAURIN himself, who engaged very warmly in this debate with the foreign mathematicians, and who, to say the truth, seems to have understood the nature of the controversy better than any one else, is frequently unguarded in his expression. In chap. II. book 2. of his account of NEWTON's discoveries, he is describing the laws of motion for the first time, and one naturally expects a more than ordinary precision and exactness. There he blames, very justly, the opposers of the Newtonian definition of motion for mistaking the direction in which the motion, lost or communicated, ought always to be estimated. But in p. 122^(c), he thus expresses himself: "When two
" bodies meet, each endeavours to persevere in its state,
" and resists any change; and because the change, which
" is produced in either, may be equally measured by the
" action, which it exerts upon the other, or by the re-
" sistance, which it meets with from it, *it follows*, that
" the changes produced in the motions of each are
" equal; but are made in contrary directions." I cannot possibly conceive, that so skilful and accurate a philosopher could believe, that the third law of motion was an

(c) Octavo edition.

inference of reason, exclusive of all experiment; and yet, if words have any meaning at all, the above quotation inclines us to think so. It is true, the change which is produced in either body may be measured by the action which it exerts upon the other, or by the resistance which it meets with from the same: but what are we to understand by action or resistance, until they are explained by more intelligible terms? or, when they are explained by terms which do not necessarily imply the same thing, how do we know that their measures are equal, or that they are made in contrary directions, until these truths be established by experiments? A law of nature is not merely a deduction of reason: it must be proved, either at once and directly, by some simple and decisive experiments; or if that cannot be done, by such experiments as enable us to collect its existence by the assistance of geometry. However obvious these reflections may appear, I thought it necessary to take notice of MACLAURIN'S assertion; because in consequence of that and similar expressions, young philosophers are extremely puzzled in the beginning of their studies, and because I have known some, who are more experienced, affirm, that the third law of motion is nothing more than a definition. I now proceed to the consideration of particular cases.

CASE THE FIRST. Suppose A and B to represent the magnitudes of two spherical bodies, and a and b their respective velocities in the same direction; suppose a to be greater than b , and A will overtake B; and if the bodies are non-elastic, they will proceed together in the same direction as one mass: if they are perfectly elastic, whatever effect has already been produced by the collision, will be repeated; and, because in the first case there is no relative velocity after the stroke, in the second the relative velocity before and after the stroke will be the same, and in contrary directions; and in either case, the motion lost by the striking body is found to be always equal to the motion communicated to B, and in a contrary direction. In this sense action is equal to re-action; and every experiment which has yet been produced, where a clear judgment could be formed of the effect, has confirmed the same thing. All the experiments which are usually brought to determine the impressions made upon soft bodies, as snow, clay, &c. are absolutely unfit for the purpose. The circumstances, which take place in the production of these effects, are such as we can never discover. The directions in which the particles recede, the velocities they acquire, their mutual actions upon one another, and lastly, the time, in which these effects are performed, are all beyond the reach of computation.

computation. The other principle, that the relative velocity of A and B is not altered by the stroke, is neither to be demonstrated nor confirmed by experience; it is a direct consequence of the definition of elasticity. Again, suppose α and β to represent the respective velocities of A and B after the stroke, and from these *data* it is easily inferred, that $A\alpha^2 + B\beta^2 = Aa^2 + Bb^2$: for $a-b$ is equal to $\beta-\alpha$, because $a-b$ is the relative velocity before, and $\beta-\alpha$ the relative velocity after, the stroke. And $Aa+Bb$ is equal to $A\alpha+B\beta$, because these quantities represent the sum of the motions before and after the stroke respectively; and from these equations the above equation is deduced, shewing, that in elastic bodies the sum of the two bodies multiplied by the squares of their absolute velocities, is not altered by the stroke.

The same theorem  may be demonstrated

geometrically in the following manner. Let the velocities of A and B be represented by AD, AB, respectively; and let G be their center of gravity, when placed at B and D; the velocity of A after the stroke will be represented by Bg, if Gg be taken equal to GD, and the velocity of B by AB+2BG. From the nature of the center of gravity $A \times GD = B \times BG$, and $A \times GD \times 4AG = B \times BG \times 4AG =$

B ×

$B \times 4BG^2 + 4BG \times AB$. Add to both sides $A \times Ag^2 + B \times AB^2$,
and we shall have $A \times AD^2 + B \times AB^2 = A \times Ag^2 + B \times AB + 2BG^2$.

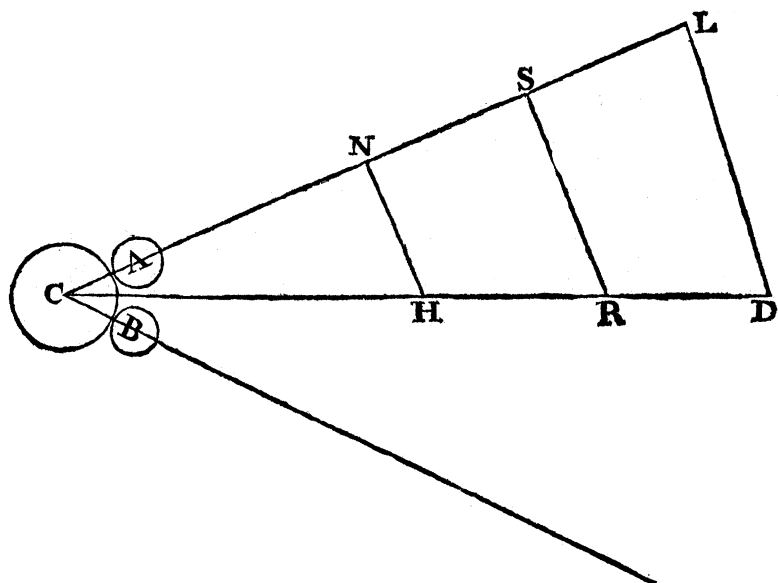
We are not to wonder, therefore, upon making trials with perfectly elastic bodies, if any such existed, were we always to find their *vires vivæ*, as the foreigners express themselves, neither increased nor diminished by the stroke. They define the force of bodies in motion, or their *vis viva*, to be in a compound ratio of their quantities of matter, and the squares of their velocities; and certainly such a definition implies no contradiction or impossibility. The term force, in a loose and ordinary way of speaking, conveys to us no determinate idea at all, and therefore, until it be defined, is incapable of being used to any good purpose in philosophy: whether this or that definition come nearer to the general sense in which it is used indistinctly enough in common language, is entirely another question. We may go farther, and add, that in their use of the words, because the sum of the forces of elastic bodies is never affected by the stroke, it is not unnatural to say, that action is therefore equal to re-action, and that no force is lost by one body but what is communicated to the other. But if we will go so far, and thereby change the meaning of the terms action and re-action and their measures, we ought at least to guard our readers from mistaking us, however

convenient such modes of expression may appear. Because $Aa^2 + Bb^2$ is equal to $Aa'^2 + Bb'^2$, it is true that no force is lost by A but what is communicated to B; but not in the same sense in which it was affirmed that no motion is lost by A but what is communicated to B. In that case the squares of their absolute velocities are understood; in this, their velocities reduced to the same direction. However, no material ill consequence can possibly arise from such a notion of action and re-action, as long as the question is supposed to concern only elastic bodies: but real mischief is done, and the debate ceases to be verbal, whenever the law of the equality of action and re-action is said to take place in the collisions of all sorts of bodies.

CASE THE SECOND. But the truth of these remarks, and the necessity of attending to the precise use of terms, will appear in a still stronger light, if we consider the solution of a problem given us by J. BERNOULLI^(d).

Suppose that two equal and spherical bodies, A and B, struck at once in the direction CD perpendicular to the line joining the centers of A and B with a velocity represented by a . Let the quantity of matter in c be called m , and the quantity of matter in A or B, n : let the velo-

(d) Discours sur le mouvement.



city of c after the stroke be represented by x , and that of A or B in the direction AC or CB by y , and suppose $p : q :: \text{rad.} : \text{cofin. LCD}$. Then, because ma the quantity of motion before the stroke is equal to $mx + \frac{2qny}{p}$, the quantity of motion after the stroke, and ma^2 is equal to $mx^2 + 2ny^2$, because the quantity of force is not altered by the collision; he easily finds $x = \frac{p^2ma - 2q^2na}{p^2m + 2q^2n}$ and $y = \frac{2pqma}{p^2m + 2q^2n}$.

There is no problem which deserves to be more considered than this by a person desirous of having a clear

idea of the grounds of that contention which has subsisted so many years. We here see BERNOULLI taking it for granted, that the quantity of force in elastic bodies is no ways affected by their mutual actions, whether direct or oblique; and the most surprizing circumstance is, that he should not so much as hint at any apparent difficulty in the present case, after he had been so very diffuse in illustrating others which were much more simple. No doubt he believed this principle to be a direct consequence of the equality of action and re-action, and therefore it is plain he could not mean the same things by those terms as we do at present. He believes no force is gained or lost by impact; he defines force by quantity of matter and square of the velocity conjointly; and in estimating the velocity, he pays no regard to the direction in which the bodies are moved. Let us not cavil at his words: we cannot mistake his meaning. The question is, how far these notions are agreeable to experience; how far they are consistent with some other principles which are incontestable, and which he himself has admitted: for instance, he admits it as an undoubted principle, that the quantity of motion in any system of bodies is preserved invariable, when estimated in a given direction, in all their collisions and mutual actions upon one another; and in this he entirely agrees with the followers of Sir

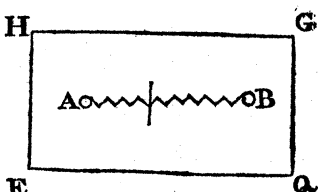
ISAAC NEWTON. Let us attend to the consequences of these two different principles in the very case proposed by J. BERNOULLI. And first, because $ma = m\bar{x} + \frac{2qny}{p}$, by transposition we have $m \times \overline{a - x} = \frac{2qny}{p}$, which is saying no more than that the motion lost by c is equal to the sum of the motions gained by A and B, estimated in the same direction cd. By a similar process from the second equation, we deduce $m \times \overline{a + x} = 2nq^2$; and therefore the comparison of the two equations gives $\frac{q \times \overline{a + x}}{p} = y$. The quantity y therefore, or the velocity of A or B after the stroke, must necessarily be equal to the sum of the two quantities $\frac{qa}{p}$ and $\frac{qx}{p}$. In the figure, let cd represent the velocity of c before the stroke, and ch the velocity after it, and let fall the perpendiculars hn, dl, upon the direction ac. It easily appears, that cn is equal to $\frac{qx}{p}$ and cl equal to $\frac{qa}{p}$, because $CH : CN :: CD : CL :: rad. : cos. LCD :: q : p$. And now the whole controversy is reduced into a narrow compass; for whether the two principles assumed by this author be consistent with experience or not; it is impossible they should be consistent with one another, unless $cn + cl$ shall be found to measure the velocity of A in the direction cl. Suppose cr to be the velocity of c after impact, when all the bodies are perfectly

perfectly hard, and letting fall the perpendicular rs ; cs will be the velocity acquired by A in that case; and, universally, the velocity acquired by A will be equal to $cs + \frac{cs}{m}$, if the elasticity of the bodies be to perfect elasticity as $1 : m$. In order to determine, therefore, when $cn + cl$ can possibly be equal to $cs + \frac{cs}{m}$, or, which is the same thing, $ls + cn$ equal to $\frac{cs}{m}$, we are to consider that $ns : ls :: 1 : m$: and because cn is equal to $cs - sn$, $cn = cs - \frac{ls}{m}$, and it is obvious that $cs + ls - \frac{ls}{m}$ can never be equal to $\frac{cs}{m}$, unless m be taken equal to unity, and BERNOULLI's hypothesis is plainly impossible in all cases where the bodies are not supposed perfectly elastic.

But though we confess the learned author, who first solved the problem we have been considering, deserves no commendation for proposing in a general form what ought to have been restrained to a particular case, yet it will by no means follow, that every argument which has been advanced against this doctrine is either intelligible or satisfactory. Of all the objections and experiments which have been started and contrived to refute the new opinions of the German philosophers, there is none which carries a greater degree of plausibility along with it, than a celebrated invention of Mr. MACLAURIN. It is
extremely

extremely simple, easy to be described; and I do not find that it has ever been answered by any of the advocates for the new doctrine of forces.

“ Let A and B be two equal
 “ bodies that are separated from
 “ each other by springs inter-
 “ posed between them, in a
 “ space EFGH, which in the
 “ mean time proceeds uniformly in the direction BA (in
 “ which line the springs act) with a velocity as 1; and
 “ suppose that the springs impress on the equal bodies A
 “ and B equal velocities, in opposite directions, that are
 “ each as 1. Then the absolute velocity of A (which
 “ was as 1) will now be as 2; and according to the new
 “ doctrine its force as 4: whereas the absolute velocity
 “ and force of B (which was as 1) will now be destroyed;
 “ so that the action of the springs adds to A a force as 3,
 “ and subducts from the equal body B a force as one
 “ only; and yet it seems manifest, that the actions of the
 “ springs on these equal bodies ought to be equal, and
 “ M. BERNOULLI expressly owns them to be so^(e). I shall
 only just observe, that if M. BERNOULLI expressly owns,
 that springs, interposed between two bodies in a space,
 which is carried uniformly in the direction in which the



(e) Book II. chap. 2. Account of NEWTON's discoveries.

springs act, will always generate equal forces in the bodies according to his own definition of that term, he talks more inconsistently than I have observed him to do: on the contrary, if I could find that he has answered this famous argument (which Dr. JURIN proposed over again in *Phil. Transf.* vol. XLIII. with a conditional promise of embracing the Leibnitzian doctrine) by simply saying, that springs he considers as motive forces, or, when the bodies are equal, as accelerating forces; and that their actions are equal, when in equal times they generate equal velocities, but not necessarily equal forces, in the equal bodies; I should not make the least scruple to own that I thought his reasoning solid and conclusive, and his distinctions a full answer to every objection of that sort^(f).

CASE THE THIRD. The two preceding cases are curious examples of the force of prejudice and party-spirit. In the latter particularly it does not appear that J. BER-

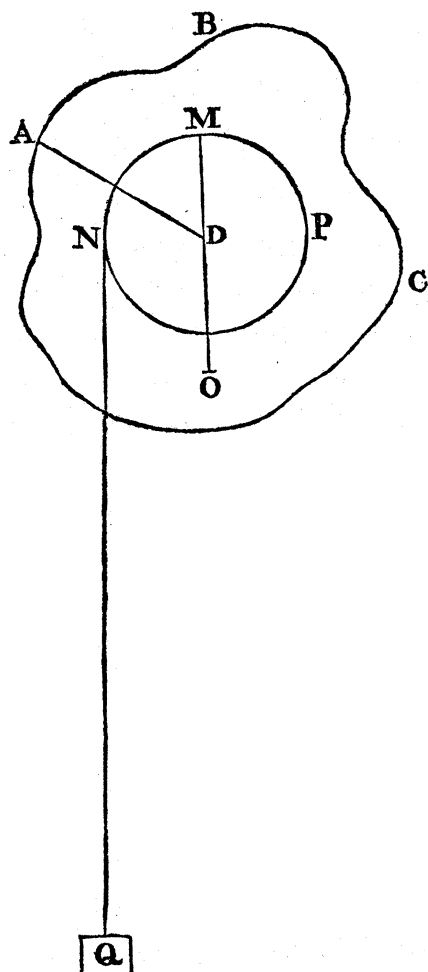
(f) No doubt MACLAURIN refers to the following passage of BERNOULLI, "La force du choc, ou de l'action des corps les uns sur les autres, depend uniquement de leurs vitesses respectives; or il est visible que les vitesses respectives des corps ne changent pas avant le choc, soit que le plan ou l'espace qui les contient soit sans mouvement, soit qu'il se meuve uniformement, suivant une direction donnée, les vitesses respectives seront donc encore les mêmes après le choc."

This quotation puts the matter beyond dispute. It is plain, BERNOULLI, though he does make use of the word action, is only speaking of the motion lost or communicated, and the relative velocities of the bodies: there is not the most distant hint at the change in their absolute forces.

BOULLI knew the preservation of the *vires vivæ* to be an infallible consequence of perfect elasticity in bodies; or indeed that he had any other reason for taking that principle for granted, but because he was not able to prove it. All the instances that are usually brought on both sides are to be treated in a similar way. The meaning of the terms must first be defined; then the principles assumed explained; and if we cannot tell at first sight, whether they are agreeable to experience or not, as is frequently the case; we must examine into their consequences by the assistance of geometry, and we shall at last arrive at some simple principle, the existence of which is necessarily implied in the original hypothesis. The collision of spherical bodies is the most simple way of communicating motion from one to another; and therefore such examples are better adapted to throw light on a disputable question, than where the suppositions are more perplexed with mechanical contrivances. Besides, when the theory of mechanics is well understood, and the foundations of error discovered, the same reasonings are easily transferred to other cases, and similar precautions applied. Indeed practical artists have little to do with the sudden communication of motion by impact. The collisions of bodies are too violent operations to enter into the com-

position of useful machines, in which motions are rather to be preserved by the gradual effects of weights and pressures. An accurate knowledge therefore of these effects is more essential to the interests of society; and the only way of arriving at such a knowledge is always to distinguish those principles which nobody denies, from those others which are found to take place only in some particular circumstances. The following problem was proposed, and a solution given to it long ago, by D. BERNOULLI ^(g).

(g) Comment. Petrop. tom. II.



“ Sit grave aliquod
 “ cujuscunque figuræ
 “ CBA, cujus centrum
 “ gravitatis sit D; ex quo
 “ et radio D M descriptus
 “ intelligatur circulus
 “ MNP, cui filum cir-
 “ cumvolutum est PMN,
 “ cujus fili extremitati
 “ appensum sit pondus
 “ Q, quod descensu suo
 “ grave CBA in gyrum
 “ agit circum centrum
 “ gravitatis D, dico velo-
 “ citatem corporis Q se-
 “ quentem in modum
 “ determinari posse. Sit
 “ $MD = a$; consideretur
 “ corpus suspensum ex
 “ puncto M oscillari, effe-
 “ que centrum oscilla-
 “ tionis in O, fitque

“ $DO = b$, pondus gravis totius CBA = P, pondus corporis
 “ appensi = p ; altitudo ex qua corpus Q delapsus est = r ;
 “ altitudo quaesita per quam grave aliquod cadendo ac-

“quirere possit velocitatem corporis $Q=z$; dico fore $z=$
 “ $\frac{apR}{ap+bp}$, et si tempus quo corpus naturaliter cadit, per
 “altitudinem R dicatur t , erit tempus infumptum a cor-
 “pore $Q=t\sqrt{\frac{ap+bp}{ap}}$, id quod experientiæ conforme esse
 “plurimis institutis experimentis semper inveni.”

Both these conclusions are derived by this author from the principle, which they call the *conservatio virium vivarum*; but as he has not given us the several steps of his reasoning, it may be useful to supply them here, before we proceed to make any remarks upon his solution. And first, suppose the axis at D to be perpendicular to the plane of the figure, and conceive the whole body to be resolved into an indefinite number of prismatic particles, each of which is perpendicular to the same plane. Let E represent the sum of all the particles multiplied by the squares of their respective distances from the axis; and E shall be equal to $R \times ab$, as is demonstrated by all the writers who treat of the center of gyration. Let v be the velocity which is actually acquired by Q after it has descended through the space R ; v the velocity which it would have acquired by the same descent, provided the body had fallen freely by its gravity; and because the *vires vivæ* are incapable of diminution or increase, we
 have

have $p v^2 = p v^2 + \frac{p a b v^2}{a^2}$. For since v is the velocity of Q at a certain period of its descent, and is to the velocity of any prismatic particle in the body as the distance $m D$ from the axis to the distance of that particle from the same, it is evident that $\frac{p a b v^2}{a^2}$ will truly represent the sum of all the particles multiplied by the squares of their velocities. v^2 is therefore to v^2 as $a p + b P$ to $a p$, and the whole force of gravity is to the force which accelerates the motion of Q in the same ratio, because in uniformly accelerated motions, when the spaces described are the same, the accelerating forces are in the duplicate ratio of the velocities. It is obvious, that the motion of Q is uniformly accelerated, because the velocity acquired by any descent is to the velocity of any point in the body always in the same ratio; and therefore the action of Q upon the body is the same as if both were at rest. Farther, the altitude z through which a heavy body must fall to acquire the velocity v is plainly equal to $R \times \frac{a p}{a p + b P}$, for the altitudes z and R are inversely, as the forces which generate the equal velocities. Lastly, the time of Q 's descent is equal to $t \times \sqrt{\frac{a p + b P}{a p}}$; because the times are always in the sub-duplicate ratio of the spaces directly, and forces inversely.

It is now extremely easy to trace these expressions back again in a contrary order, and to shew, that if these last equations are true, the original one must be true also; that $p \times v^2$ must necessarily be equal to $\frac{pabv^2}{a^2} + p v^2$, or, which is the same thing, that the body Q multiplied into the square of its velocity, and added to the sum of all the products which arise by multiplying every particle into the square of its respective velocity, is equal to the body Q multiplied by the square of the velocity which it would have acquired by the same descent *in vacuo*.

Now this is to give the argument its full force; and since the conclusions are confirmed by repeated experiments, as the author himself assures us, it is presumed, that the premises can be liable to no just exception. If we do not think with the advocates for this doctrine, that the *vires vivæ* must always remain the same from the thing itself, they will force our assent by the testimony of experience, and oblige us to admit their principles when we find it impossible to deny the consequences.

A prudent philosopher is always afraid to pronounce generally concerning the existence of causes, which are attended with a variety of circumstances, and are complex in their operations. To say that the quantity of force in bodies remains invariably the same, seems to be
a propo-

a proposition of this kind. The mutual actions of bodies upon one another, especially when their gravity is taken into the question, depends upon so many considerations, and the cases which may be put are capable of such an infinite variation, that it is impossible almost to draw a general inference of this nature. Even when experiments are produced, which seem to prove the point, one is apt to suspect the universality of the conclusion, and to imagine that it may possibly be owing to some particular circumstance which we have not attended to, or been able to distinguish from others not so essential. In the example we are considering it is clearly proved from experience, that $p \times v^2$ is equal to $p v^2 + \frac{E v^2}{a^2}$; but whether that be true in every other case that may be conceived, can never be determined from such an experiment; nor is it possible to make any distinctions about it, until we have demonstrated its connexion with some other principle, which is more simple and less contested.

Retaining the same symbols, let F represent the force of gravity, and f the force which accelerates the body Q in its motion. From what has been already shewn it appears, that $F : f :: ap + bp : ap$ and $F - f : f :: bp : ap :: \frac{abpv}{a^2} : pv$; and because $p v$ is the motion generated in Q

by the force $f, \frac{abPv}{a^2}$ will be the motion lost in the same body Q by the diminution of its gravity. Let A be any prismatic particle of the body, and AD its distance from the axis; the velocity of this particle will be $\frac{v \times AD}{a}$; its motion $\frac{A \times AD \times v}{a}$, and, by the nature of the lever, the motion which Q must lose to generate such an effect in A must be $\frac{A \times AD^2 \times v}{a^2}$. The quantity $\frac{vabP}{a^2}$ represents the sum of all the quantities $\frac{A \times AD^2 \times v}{a^2}$; and therefore the motion, which Q has lost by its action on the body, is precisely equal to the motion gained by the different parts of that body after a proper allowance is made for the lengths of the levers, AD, &c.

Thus it appears, that there is no necessity in accounting for the time of Q's descent and the velocity it acquires, of having recourse to the *conservatio vis vivæ*, or any such perplexed hypothesis. By pursuing the analytic method far enough, we have been led directly to that fundamental law of motion, that action is equal to re-action, and in the contrary direction.

A distinction, however, is always to be made between the actions of bodies when at liberty, and when they revolve about a center or axis. In the first case the motion
lost

lost is always equal to the motion communicated in an opposite direction: in the second the motion lost is to be increased or diminished in the ratio of the levers before it will be equal to the motion communicated. The properties of the lever are well understood and easily applied, and because their evidence depends upon experience, and is as firmly established as the third law of motion itself, it is always best to make use of those two universal principles, instead of others which are more liable to deceive us^(b).

In all cases concerning the motion of a single body, or system of bodies, where there is any rotatory motion, the consideration of the lever becomes requisite, and that, with a just application of the laws of motion, is sufficient for the resolution of the most arduous problems. It is

(b) It is acknowledged, that the experiments which have been made to determine the effects of wind and water-mills do not agree with the computations of mathematicians; but this is no objection to the principles here maintained. Writers generally propose such examples with a view rather of illustrating the methods of calculation by algebra and fluxions, than of making any useful improvements in practice. They suppose the particles of the fluid to move in straight lines, and to strike the machine with a certain velocity; and after that, to have no more effect. As such suppositions are evidently inconsistent with the known properties of a fluid, we are not at a loss to account for a difference between experiment and theory; and therefore it should seem unreasonable to assert, that certain authors of reputation have neglected the collateral circumstances of time, space, or velocity, in the resolution of these problems, unless we were able to point out such omissions.

now pretty well agreed upon, that the neglect of this circumstance is one cause of that material error, which Sir ISAAC NEWTON himself is supposed to have fallen into in the thirty-ninth proposition of the third book of his Principia.

I had several reasons for insisting so particularly on the demonstration of this third case. It is in itself one of the most neat and elegant problems we have; and, what is of more consequence, it admits of an experimental proof and illustration. It is obvious, that the motion of the body AMB may be made so slow, that the time of Q 's descent through any assignable space may be measured to the greatest exactness. The velocity of Q may also be inferred with the same ease by observing the velocity of any particular point in the body to which the velocity of Q always bears an invariable ratio. Such experiments, it must be owned, seem very unfit for the first discovery of the laws of nature; though, as I have shewn, it is not impossible to collect them that way; but after they are discovered, the application of them to the solution of such intricate problems is both entertaining and instructive, and then the agreement of the experiments themselves with the theory becomes a solid argument for the certainty of our principles.

We have shewn, that in this case at least BERNOULLI's hypothesis is founded upon, and coincides with, the commonly received doctrine of motion, and therefore we can hardly entertain a doubt of the success of the experiment, supposing it had never been tried. The author himself, in the passage above quoted, tells us, that he found it so; but we need not rest upon his authority: a similar experiment has been lately made by Mr. SMEATON, and is described at length in the *Philosophical Transactions*, vol. LXVI.

It does not appear, that D. BERNOLLI attempted to measure any thing but the time of Q's descent through any particular space: Mr. SMEATON has given us both the times of Q's descent, and the proportions of the velocities acquired, in a variety of cases. By moving the weights he makes use of nearer to, or farther from, the center D, he alters the lengths of the levers at which the particles act, without increase or diminution of their number: he does the same with the circle or axis NMP, and consequently the lever MD; and in every case, from the known character of that ingenious gentleman, we may presume that his numbers are safely to be relied upon.

His conclusions may receive some illustration from the preceding theory.

From the proportion $F : f :: ap + bp : ap$, it appears, that the force which accelerates the motion of Q , or in Mr. SMEATON's figure, the weight in the scale is to the natural force of gravity in a constant and invariable proportion as long as the quantities a , b , r , and p , remain the same; and therefore let Q descend ever so slowly, its motion will be uniformly accelerated throughout, and the spaces through which it descends will be as the squares of the velocities acquired, and the times will be as the velocities themselves; and this is agreeable to what Mr. SMEATON found them in his second and third, fifth and sixth, eighth and ninth experiments.

The general expression for the force which accelerates the weight in the scale is $\frac{ap + bp}{r \times ap}$, and will be different according as the quantities a , p , or b , are altered; but is always easy to be determined as soon as those quantities are known. But it is impossible to determine the magnitude of the quantity b in the different cases, unless we have given the precise dimensions of the whole machine, and the specific gravity of the wood made use of; and therefore I confess myself to have been puzzled in endeavouring to reconcile the first and second and other experiments with the theory: for though I could not doubt a moment, that the general expression for the force was
rightly

rightly affigned, and would always be found confonant to experience, yet I was extremely furprized to find, that when the quantity a in the fecond experiment was made exactly one-half of what it was in the first, the time of defcending through the fame space came out nearly double of what it was before, and the velocity the fame. Now this I knew could never happen unlefs the force in the first cafe was to the force in the fecond as 4 to 1; for when the fpaces described are the fame, the accelerating forces are always as the fquares of the velocities, or inverfely, as the fquares of the times. This confideration led me to inquire farther into the ratio of thofe forces in the cafe described, in order to difcover, if poffible, whether they came any thing near that ratio, which of neceffity they ought to do.

I confidered, that the weight of the axis and arms of the machine was inconfiderable, compared with the weight of the two cylinders of lead, and alfo that the quantity a bore a very fmall proportion to the length of the cylindrical arms of fir. And fince the accelerating force is always as $\frac{ap}{ap+bp}$, or as $\frac{a^2p}{a^2p+abp}$, and the quantity abp or E expreffes the fum of all the particles multiplied by the fquares of their diftances from the axis of motion, it is plain that E muft far exceed a^2p ; and, laftly, fince the quantity E is

the same both in the first and second experiment, it follows, that the forces are very nearly to one another as a^2p to $\frac{a^2p}{4}$, or as 4 to 1: and in the same way the other experiments are shewn to be consistent with the theory.

I chose to premise a short account of the opinions which the philosophers before GALILEO entertained concerning the motions of bodies; because their mistaken ideas of the effects of gravity are analogous to some opinions of a later date, which indeed suggested the necessity of reforming these inquiries.

And as nothing in controversial matters so completely satisfies the mind as an exact knowledge of that particular which produces the dispute; I have shewn, that the terms made use of to express the third law of motion were taken in two very different senses: that Sir ISAAC NEWTON's explication of them is at best ambiguous, and MACLAURIN's absolutely false.

1st. In the demonstration of the first case we see that the assertion of LEIBNITZ is true in one particular instance. When two elastic balls move in the same straight line, the sum of their forces is not altered by collision; and it is more than probable, that this single circumstance was the cause of affixing new ideas to the terms action and re-action. For,

2d. In

2d. In the second case, the same principle is taken for granted by J. BERNOULLI. We have examined into the consequences of this author's solution, and shewn that his hypothesis will prove all bodies to be perfectly elastic. As the steps by which he deceived himself are here exposed, whoever carefully attends to these two examples cannot easily mistake in any case that may occur. It is plain, that if any one contends for the equality of action and re-action, and explains those terms by the changes produced in the absolute forces of the bodies, the dispute is not merely verbal.

3d. When a conclusion, agreeable to experience, is deduced from any hypothesis, it does not therefore necessarily follow, that the hypothesis is universally true, not even supposing the converse of the proposition to hold. In this third case it is shewn, what kind of answer we are to give such reasoning. The *conservatio virium vivarum* is never to be admitted, unless its connexion with simple facts, which are incontestable, be first made out. The solution of this problem depends on this, that the motion lost is equal to the motion communicated in a contrary direction after the property of the lever is taken into the account; and therefore the nice agreement of Mr. SMEATON's experiments with the
theory

theory cannot fail to add fresh evidence to these established laws of nature.

I shall conclude these remarks with observing that since it is perhaps impossible to give one general answer to all the arguments which are brought in favour of the new doctrine of forces, it seemed very desirable that we should have a general rule to direct us in judging of the cases that occur in practice. It is of more consequence to the improvement of science and the good of the public, to point out the source of mistakes, and the wisest means of avoiding them for the future, than merely to confute and silence our adversaries. Some writers have considered this question as entirely verbal, and have affected to treat the advocates on both sides with the greatest contempt. Such persons save themselves a great deal of trouble, and have the credit of seeing farther into the controversy than others; but after all, I am afraid the practical mechanic will receive little information or security from such speculations. Propriety of expression in these matters is not all we want. When a plan is proposed for execution, and a certain effect predicted, the grand object is, how to form a sure judgement beforehand of the event, in order to prevent unnecessary expences; and I shall think my time well employed, if these
confi-

considerations appear to have the least tendency to promote so useful an end, in the opinion of that Society to whose learned and zealous endeavours we owe the very first important discoveries in the year 1668, concerning the collisions of bodies.

